

June 17, 1943

Dr. S. E. Luria
Department of Biology
Princeton University
Princeton, New Jersey

Dear Luria:

I'm sorry to say I have heard of no openings for the girl you mentioned. Under the present circumstances, one would suppose that there are many places heading additional now where she would be most welcome, if she is moderately good; but I have heard of none.

There is, however, an opening at the Marine Biological Laboratory this summer for a teacher in the Invertebrate Zoology course. If she is well prepared in that subject she might apply to Dr. John Buck of the Department of Zoology, University of Rochester, Rochester, New York. Buck wrote me inquiring for leads. The job would last only for a short time; from July 27 to August 31st. So I'm not sure your friend would be interested, though it might well provide her with contacts for further openings.

I think you need not worry at all about the appointment for next year. I do not believe the delay has anything to do with Cleland's illness. None of the appointments for next year have yet been made so far as I am aware. The delay is probably due to the fact that the board of trustees has not yet met to pass on these matters.

Cleland sent me the manuscript of your paper, for he knew I would be interested in it, which of course I was. I felt that the experimental work was completely unobjectionable and excellent, but it seems to me there are some serious objections to the theoretical part of the paper. The basic assumptions on which the theory is developed appear to me to be unsound. In the first place, I do not agree that there are only two possible interpretations to be distinguished. Secondly, I think that the mathematical relations would hold equally well for other interpretations than the mutation one. All that is shown is that the changes in the bacteria are more often found between closely related cells than between more distantly related ones. I have seen what seem to be the same or very similar relations in my work on acquired resistance to specific antibodies in *Paramecium* and proved beyond question that the changes were not mutations, though they were inherited for hundreds of asexual generations. I believe I sent you an abstract of the paper. I hope to write the full paper some time this summer. The critical test of inheritance at sexual reproduction was the one that showed the changes were not mutations. I

Sonneborn mss. II
Manuscripts Department, Lilly Library
Indiana University, Bloomington, Indiana

For Reference Use Only. This Material
is covered by copyright and may not be
quoted or reproduced without permission
of copyright holder.

also showed that the changes in Paramecium were definitely due to the action of the antibodies. Yet they were not producible at random as you assume must be the case; they were producible only in certain clones and, within these clones, only on individuals in a certain recognisable physiological state. Finally, I don't see that you have shown, or even made reasonably probable, the independence of the changes in the bacteria from the action of the virus.

I hope you will remember to talk this over with me when you return, for we can deal with the questions much more satisfactorily in conversation than by correspondence.

Best regards and wishes for a pleasant and successful summer in spite of the bad weather (we have the same here!)

Cordially yours,

T. M. Sonneborn

TMS:fr

Sonneborn mss. II
Manuscripts Department, Lilly Library
Indiana University, Bloomington, Indiana

For Reference Use Only. This Material
is covered by copyright and may not be
quoted or reproduced without permission
of copyright holder.